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AN
ESSAY

ON THE

VITALITY OF THE BLOOD.

BY

JAMES CORRIE, M. D.

“ But flesh with the life thereof, which is the blood thereof, shall
“ you not eat.” GENESIS, ch. ix. v. 4.

Dr. CAIUS.—“ Sir John, you would do well to lose little blood.

FAL.—“ Me! I thank thee. But in the blood is the life of the crea-
“ ture; and I will not consent to part with mine.”

FALSTAFF'S WEDDING, act iii. sc. ii.

L O N D O N :

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1791.

UNIVERSITY
OF BRISTOL
MEDICINE

TO

JOHN HUNTER, Esq. F. R. S.

*Surgeon Extraordinary to His
Majesty, Surgeon General to the
Army, &c.*

S I R,

AS soon as I resolved to prepare a Thesis, the Vitality of the Blood was the subject most forcibly impressed on my mind for that purpose; but at that time it had scarcely entered my imagination that I should ever have an opportunity of seeing or knowing the celebrated author of that opinion personally.

But ever since I commenced the
study

study of medicine, my intentions have been to set such an eminent pattern before my eyes, and to endeavour to follow, though at a great distance, such a praise-worthy example; and whenever I determined to publish, in its present form, the following little Essay, I resolved, and I think with propriety, to lay it before you, to whom I owe the first and leading principles thereof; sensible at the same time, that no dedication whatever can add to your fame, it stands on a much more stable foundation.

I am also well convinced, that I never could submit this juvenile
per-

performance to a more capable or more impartial judge; and wherever I have been led into any error or mistake, (for mistakes, no doubt, there are) I shall be happy to be corrected; and where it may appear I have followed the right path, if it merits your approbation, it will give the utmost pleasure to,

S I R,

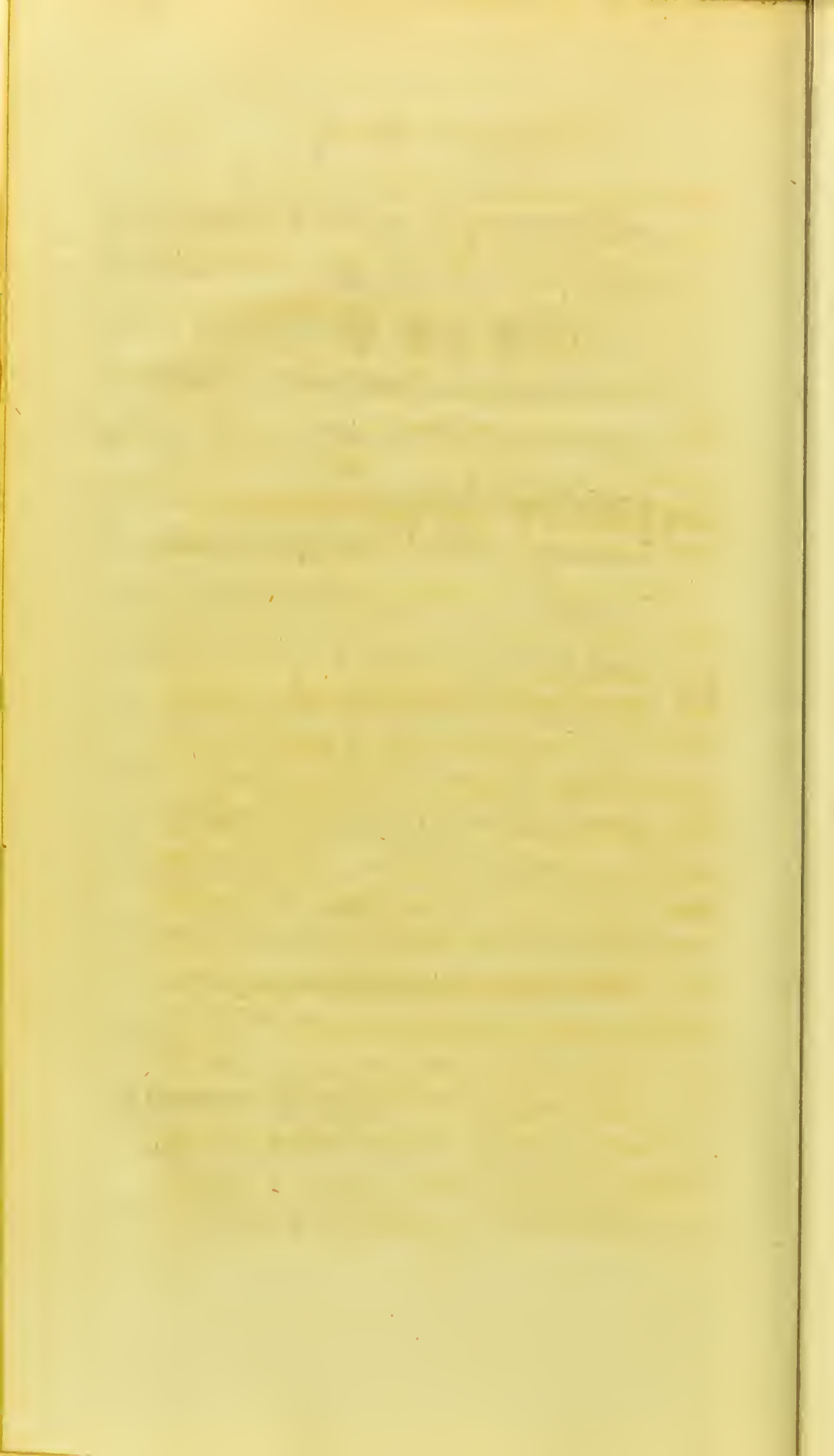
Your most obedient,

Most humble,

And devoted servant,

JAMES CORRIE.

LONDON, MAY 25, 1791.



AN
ESSAY
ON THE
VITALITY OF THE BLOOD.

IN composing the following essay I had several incitements; but disappointed in some of them, I have, with the advice of a few friends, ventured to lay my thoughts on this subject before the public in the present form; not supposing that much new matter will be found therein, but that the attention of men of learning and experience, may be drawn to a subject undeservedly buried in oblivion; for while it was universally allowed that the animal machine, although acknowledging the influence of the common laws of matter, was governed by a principle peculiar to
B life,

life, yet this principle was never till lately supposed a property of the blood of animals: for although in the works of the immortal *Harvey*, we find some loose hints which may lead to the conclusion, that he supposed the blood of animals possessed of a living power, yet the doctrine, it would seem, had made few converts, and was, I may say, entirely discarded, till that truly able philosopher Mr. John Hunter of London, called the attention of physicians to it not many years ago; since which, some have laboured not a little to combat the opinion, while another as strenuously defends it, even from sacred writ.

How far my experiments and reasoning corroborate the opinion, does not become me to judge; with diffidence I present them to the public, and claim that indulgence which is due to a very young man.

As most of the experiments were unavoidably made on a small scale, the result may, in many cases, when repeated on a larger, be somewhat different; yet I presume they will be found sufficiently accurate for ascertaining the end I had in view. No doubt inaccuracies may have crept in,
and

and that either from accident, or my thinking I saw that which I wished to see; which last is often the case with those who feel a warm affection for a child of their own fancy.

In several places I have purposely avoided recommending practices which result as a consequence of the doctrine inculcated; leaving that to men of greater experience and depth of judgement; and upon the whole, although we have failed of producing a clear conviction, yet from the collision of facts, sparks I hope are thrown out sufficient to shew a future adventurer his way.

Having necessarily, as I think, premised this much, I proceed to state the order in which I mean to treat of this subject; and that which at present appears to me the best will be:

1st. To shew that fluids, as well as solids, can admit of animation.

2d. To define what I understand by the animation or life of the blood.

3d. To prove that, in the sense adopted, life exists in the blood.

4th. And lastly, endeavour to determine the source from whence the blood derives its life.

To prove the first position, naturally occurred as the first step of this enquiry; since some have taken upon them to assert, that in even the nature of things, blood cannot be alive; because, being fluid, it does not admit of organization; life, as they say, consisting in the performing of certain functions of an active kind; and for the performance of these functions, organization they view as absolutely necessary*.

In a different light, however, it appears to me; for the only real difference betwixt a solid and a fluid seems to consist, in the particles of the latter being more moveable the one upon the other; we are also accustomed to view different bodies in both states, and from these alone, may we not affirm, “that there is not a more intimate connection between life and a solid, than life and a fluid.”

* Dr. Hendy's Essay on Glandular Secretion, p. 70.

To confirm that, however, we can go farther, and as the works of nature are very obscure, I do not see why a certain obscure organization may not subsist in the blood of animals, that does not come within the reach of the evidence of our senses, and even although in that state, not the less real; for would any person take upon them to deny, the organization of the embryo, in its early state, because it is not obvious to our senses.

Farther it appears to me, that what Dr. Hendy defines life to be, is only a consequence of life, conjoined with a certain organization, and that from either a want of organization, or difference thereof, the effects this life shew, acted upon by the same cause will be different; and therefore although we do not find the same phenomena follow, from the same cause acting upon muscular fibre and blood, we are not at liberty to suppose the blood dead, since different phenomena evidently result, from the same stimulus acting upon evidently organised living matter.

But although different phenomena result, from the application of the same
cause

cause to organised living matter, yet one general law affecting all living bodies is, that they have a tendency to resist the action of those bodies which threaten their destruction. This is undoubtedly the effect of life in every case; and hence, if we can find an animal substance devoid of apparent organization, possessed of such a power, we are irresistibly led to infer, that the blood may, for any thing we know to the contrary, be possessed of life.

That there are animal substances, devoid both of apparent organization and spontaneous motion, possessed of a power of resisting the action of hurtful bodies, every one will be perfectly convinced of, when he considers even the phenomena of incubation.

Experiments have shewn that the egg, which comes undoubtedly under the description, is possessed of a considerable power, both of resisting heat, cold, and putrefaction, and that too in a degree nothing inferior to many of the less-perfect animals.

To prove the first part of this assertion,
that

that the egg, although deprived of visible organization and spontaneous motion, is capable of resisting, for a length of time; heat, we have only to mention what happens during the process of incubation, where in that of the hen, it is situated in a heat of 103 deg. F. scale for three weeks, and in the same heat in the ducks for four; yet the yolk, which is not diminished, is always perfectly sweet to the very last; also that part of the albumen which is not expended on the growth of the animal, some days before hatching is also perfectly sweet. But if the egg is not hatched, it becomes as certainly putrid, in very near the same time, as any other dead animal matter.

But eggs stand more direct proofs of a living principle, by resisting of cold, the second assertion. Various experiments of Mr. Hunter's confirm this*; all of which I have had the curiosity to repeat, and found to correspond so, that I could easily account for all the difference, which was small, from a difference in the thermometer. For a full account of them, I refer

* Hunter's Animal Oeconomy, p. 106.

to Mr. Hunter's work itself: here I will barely mention, that from them we find, that by freezing an egg, thawing it, and then placing it in a cold about (O) (being that temperature in which it was formerly frozen) along with another new-laid egg, we find that the former freezes nearly ten minutes sooner than the latter.

We also find that an egg froze, in an atmosphere fluctuating between 15° and 17° deg. above (O) then thawed and exposed in an atmosphere below freezing, yet nearly ten degrees warmer than that in which it was formerly froze, will freeze in one half the time it did before.

And finally we find, that when an egg, which has been once frozen and thawed again, and a new-laid egg, are put into a cold at or about 15° above (O), that the thawed one soon comes to 32° , begins to swell and congeal; while the fresh one suffers its heat to be diminished to even 29° or 30° , and remains in that state for nearly twenty minutes; after which it suddenly rises to 32° , the freezing point, and begins to swell and congeal.

That

That these few simple experiments prove our position cannot but be obvious to every observer ; for unless there were attached to the fluids of the egg a principle of life, how could it resist the action of such a heat for so long a time, as, during the process of incubation, this property being only peculiar to living bodies ; for when this principle is destroyed in the egg, and situated under similar circumstances, it becomes putrid nearly as soon as any other dead animal matter ; and why not as soon as other dead animal matter, is, perhaps, owing to the less free action of the air. The other three sets of experiments taken conjunctly, prove that cold, when applied in certain degrees to eggs, has a power of destroying some principle which has a tendency to resist the action of said cold : hence life ; for after its destruction by a freezing process, they no longer resist the cold, but freeze in one half the time they did before, which certainly would never take place, were not some principle destroyed or expended in the former freezing process, while endeavouring to resist that which threatened its destruction.

Having thus, then, determined that the egg, an animal substance, evidently possessing neither visible organization nor spontaneous motion, is possessed of a principle of life; farther proof is not necessary to confirm the idea that fluids can admit of a principle of life; yet we can carry the point farther, and, as I said, clearly shew that these substances possess this principle, in a degree nothing inferior to many of the less perfect animals.

That this is true, a set of experiments, no less happily instituted than judiciously executed by Mr. Hunter, fully confirm *; but for particulars I refer to the work itself; here I will only mention in the most cursory manner, that having placed vipers, frogs, eels, snails, and leeches, in a degree of cold similar to what was used in the experiments related on eggs, that they suffered their heat to be diminished below the freezing point similar to the egg, and having continued in that situation for nearly the same length of time, they generally then rose to 32° , the freezing point, and congealed; by which

* A Oecon. p. 104.

process, after they were thawed, they were generally found to be dead, more especially in the snails and leeches.

Here, then, we find that the same cause, applied in similar circumstances to animals, as well as animal substances devoid of apparent organization and spontaneous motion, produce the same effect; hence we cannot but infer, that the principle which causes both is the same. The same effects are also shewn from the application of another cause; for while, as experiments shew*, vipers and frogs possess a considerable power in resisting heat, yet they evidently have it not in a greater degree than the egg; while this last, as would appear by another experiment, evidently has a power in this point superior to animals not the least imperfect; for it appears that upon putting a living and a dead eel, and a living and dead tench into warm water, all of them receive that heat alike quick; and when again exposed to the cold, part all of them alike quick with it; while by

* Hunter's Animal Oeconomy, p. 105.

experiment it is found that an egg, when placed in warm water, resists the heat for some length of time, and more particularly when new laid; and I have been informed by an experienced cook, that a new-laid egg requires considerable more time to boil, than one which has been laid for some time, which I could only explain by supposing the power of resistance greater, and consequently the principle of life more vigorous.

These facts and observations, then, of Mr. John Hunter's, teach us, that animal substances, devoid of apparent organization and spontaneous motion, are possessed of powers capable of resisting heat and cold, nothing inferior to many of the less perfect animals, and as a natural consequence of the former putrefaction also; for it is only by animals, or animal substances, being possessed of life, that they resist, when placed in certain situations, putrefaction. Hence, when we find that this power, which resists the putrefaction, is as easily destroyed by heat or cold, in some animals, as animal substances devoid of apparent

rent organization and spontaneous motion; we can but infer that the one resists putrefaction as powerfully as the other, since the cause which enables them to resist this putrefaction is alike easily destroyed in both.

Having thus, then, controverted the general law of Dr. Hendy, and shewn that fluids can possess a principle of life, and hence that the blood, for any thing we know to the contrary, may possess this principle, we come to the second great head, the defining what I understand by the life of the blood : previous to which, however, it is necessary to remark, that we are entirely ignorant of what this principle in substance consists of ; it is only from its effects we can judge of its presence ; for while attached to matter, that matter, besides acknowledging the influence of the common laws of matter, is governed by a principle peculiar to life. Hence our definition will not consist of what life is, only the effects which this life shew when acted upon by other agents.

The definition which follows is the best of any which has as yet occurred to me ; I am far from thinking it perfect ;
future

future observation may, perhaps, lead to this ; at present for want, we content ourselves with it, conscious, however, that if we prove every part of it, imperfect, perhaps, as it is, we infallibly establish a living principle in the blood.

DEFINITION.

By life, then, in the blood, I understand that property in it, by which, while attached to it, it is rendered incapable, when extravasated, of acting on the living solid as dead matter ; by which it is sensible to the application of a stimulus ; by which it resists the action of those causes which tend to its destruction ; and that principle which is the cause of all the actions that it induces.

This definition is naturally divided into four sections, each of which I will briefly endeavour to prove, which leads to the third great head I have marked out, and to which I shall immediately proceed ; and on the proving of which depends the truth of the whole doctrine.

That the blood, then, is possessed of a
pro-

property by which, while attached to it, it is rendered incapable, when extravasated, of acting on the living solids as dead matter, I will endeavour to prove by shewing, that from its application to them, the same effects do not follow as follow that of dead matter. Here in this part we are led to enquire, whether the effects which the blood shew be different when contained in its proper vessels, or when extravasated, and consequently unconnected with the source of life. To me, in both cases, it appears the effects are the same, so long as this principle remains attached to the blood; but as the consideration of the effects of the blood, when flowing in its proper vessels, is more properly comprehended under the fourth head, we wave the enquiry as to it, and only endeavour to determine what the effects are of the blood extravasated, and applied to the animal living solid; previous to which we will mention what the effects are which follow the application of dead or inanimate matter, by which we shall be able to ascertain if the blood can be comprehended under that class.

The

The effects which follow the application of dead matter to the animal living solid are so well and universally understood, that it is scarcely necessary to investigate it very particularly, it being evident, that by their action a particular process is brought about, which either terminates in the throwing off the dead part, or, if such part was previously inserted betwixt the lips of a wound, so that nature could not effect its removal; so long as it remains in that situation no union of parts will follow. These are the constant and invariable occurrences which follow the action of dead matter on living bodies; but the action of the blood is different; consequently we can but infer that blood is governed, in this particular, by laws different from those of inanimate matter, which were it not the case, as blood is commonly, in a greater or less degree, inserted betwixt the lips of every wound, no union of parts could take place in the animal kingdom. Agreeable to this we find that one of Mr. Hunter's arguments is *, "that it unites living parts in some cir-

* Ed. Medical Commentaries.

" cum-

“ circumstances, as certainly as the yet recent juice of the branch of one tree unites it with that of another :” and, says he, “ was either of these to be considered as extraneous or dead matter, they would act as stimuli, and no union would take place either in the vegetable or animal kingdom.”

The truth of this argument is, from what has already been said, sufficiently evident ; yet Mr. Hunter, to render it still, if possible, free from objection, states an experiment made on the testicle of a cock introduced into the abdomen of a hen ; the former of which, while bloody, he introduced into the abdomen of the latter : upon killing the hen some time after, and injecting her, he found that he injected the testicle also, which had adhered to the liver. Dr. Hendy, in his Essay on Glandular Secretion, (a work replete with much useful information), and his follower, Dr. Adye, in his inaugural Dissertation de Sanguinis Circuitu, take upon them to combat the idea of life in the blood as introduced by Mr. Hunter. Their reasoning, however, will in many places, I trust, be found unphilosophical,

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and not strictly just: but as Dr. Hendy was the first to impugn Mr. Hunter's doctrine, and has brought almost all the arguments himself against it, his follower only repeating them, we will direct our chief attention to him; and here we shall find that those objections, which he brings against this argument, is a mere sophism. Mr. Hunter having used the word extraneous, Dr. Hendy waving the argument, sensible of its force, catching at words, merely proposes (in my opinion to the end that he may divert the attention) the following question: "Why did not the testicle of the
" cock act as dead matter, when it came
" in contact with the liver, and produce a
" particular process, according to Mr.
" Hunter's own idea, which would terminate in the throwing off the dead or *extraneous* part from the living or sound
" parts?" Hendy here also waves the enquiry, but not with justice, whether the testicle was alive or no, merely saying, that it was certainly extraneous as to the liver of the hen, and ought, according to Mr. Hunter's own idea, not to have adhered thereto. Dr. Hendy's explanation of the word extraneous is, perhaps, here literally
just;

just ; but then it is evident that Mr. Hunter uses the word extraneous to express a matter foreign to animal life, and evidently devoid of animation ; while this is the case then, and while the effects are constant from the application of such a cause, no such effects following the action of the testicle besmeared with blood, we are irresistibly led to the conclusion that both, in every sense of the word, are possessed of life. That the former was alive, although deprived of sensation, is evident from what we see happen to it when either stimuli, chemical or mechanical, are applied, and also from analogy, where we find that other parts cut out of animals can be acted upon by stimuli, even for a length of time, and in some cases even the separated part has the power of becoming an animal perfect in all its parts ; and, as we said before, we cannot but admit the blood to be alive, since it has not the effects of other dead matter ; it is, indeed, impossible to conceive that were it dead, vessels could shoot through it, for it would be as effectual a bar to their elongation as a piece of bougie, or any other inanimate matter, which we

often see used for that purpose : moreover, if I may be allowed the expression, so far from considering it dead matter, I view it as the vinculum, uniting all those parts which adhere by the first intention, as surgeons speak, and, as such, we cannot but admire provident nature ; for by its insinuation betwixt parts divided it effectually excludes air, than which nothing is more powerful in producing violent inflammation, and all its consequences, by which the healing of wounds would be very much protracted in many cases. In such a situation blood, besides answering this useful purpose, evidently appears from experiment possessed of another property ; for while it thus glues, as it were, parts, and effectually excludes the introduction of inanimate matter, it besides possesses a power of generating vessels, and thus rendering the adhesion perfect and complete, which at first was only temporary, and liable from slight accident to a disunion. This leads to the consideration of another of Mr. Hunter's arguments, which is, “ that blood
“ becomes vascular like other living parts,
“ for the coagula in the extremities of arteries,
“ ries,

“ries, after amputation, may be injected
“by injecting these arteries.” Dr. Hendy
admitting the verity of this argument, yet
affirms, “that although this be the case,
“nevertheless the fluids which these ves-
“sels carry may not be alive; for (says he)
“as the stump inflames, and as we have
“shewn inflammation disposes the vessels
“to elongate, by this they are elongated,
“and pass through the coagulum, and
“anastomose; but they by no means
“take their origin from the coagulum.”

To these arguments of Dr. Hendy's, al-
though spacious, I would only observe, that
admitting inflammation disposes the vessels
to elongate, how, as I have already said,
can we suppose them to pass through dead
matter; for if instead of the coagulum of
blood we insert a substance, to appearance
the most analogous, the coagulum of wheat,
into the mouth of an artery, we shall, to be
sure, find inflammation follow; but we
shall never be able to inject vessels shooting
through this dead body; on the contrary,
the inflammation induced would occasion
such pain and consequences as we see fol-
low the shooting of inflamed vessels into the
inter-

interstices of sponge, when used to stop hemorrhage. Besides Mr. Hunter has, he informs us, a preparation in which he can demonstrate vessels rising from the center of a coagulum of blood, which entirely invalidates the last part of Dr. Hendy's objection, and tends much to confirm that, in the experiment on the hen, vessels did arise from the coagulated blood. Moreover, what to me tends to confirm the opinion not a little is, that in those cases of wounds which heal by the first intention, a very slight degree of inflammation occurs, even in some cases so slight as to be almost imperceptible; hence then, unless we suppose in the blood a tendency to generate new vessels, how in such a case does adhesion take place, since we find the only power (as they say) which unites parts is wanting.

Notwithstanding these strong proofs, Dr. Hendy even denies the possibility of vessels taking their origin in coagula of blood, otherwise he would expect to find them in those clots that are termed false conceptions, but which is by no means the case. To this I would say, that although
vessels

vessels have never been demonstrated in such clots, it is no proof that they do not exist: but setting aside this, I think I may affirm that these clots are possessed of a certain degree of animation, otherwise how could they resist the action of such a degree of heat conjoined with moisture without becoming soon putrid; therefore they must be possessed of a certain degree of life, as living bodies alone have this power; for we see that when a child dies in utero, which is by no means a rare occurrence, it becomes soon putrid: hence were these clots dead matter, they ought, being placed in similar circumstances also, to become putrid.

Here Dr. Hendy, finding what he thinks some seeming inconsistencies in Mr. Hunter's arguments, with an air of importance endeavours to turn them against him. I will attempt to shew, however, but with a becoming modesty, that he impugns a doctrine, without being fully master of the arguments; for, says he, "blood
" in its coagulated state cannot possibly be
" said to possess life, according to Mr.
" Hunter's own idea of life, who, in con-
" sidering,

“ fidering the blood as alive, views it as a
“ fluid, as is evinced by his alledging, that
“ in the nature of things there is not a more
“ intimate connection between life and a
“ folid, than life and a fluid.” To which
he adds, “ as the blood by coagulation lofes
“ its fluidity and becomes folid, and as Mr.
“ Hunter feems to admit that this coagula-
“ tion is a morbid change produced by the
“ ftimulus of expofure,” hence he con-
cludes, “ admitting that the blood whilft
“ fluid was really alive, that it muft, ac-
“ cording to Mr. Hunter’s own words, be
“ confidered, when in its coagulated ftate,
“ as being really and certainly dead.”

To thefe arguments, in the firft place, I
would obferve, that although Mr. Hunter
fays, that there is not a more intimate con-
nection between life and a folid than life
and a fluid, he does not fay that the blood
lofes life when it lofes its fluidity ; on the
contrary, he affirms, “ we often fee the
“ fame body fluid in one cafe, and folid in
“ another : ” * and certainly all that can
be drawn from his arguments is, that the

* Med. Com. vol. II. p. 199.

life of the blood, in its fluid state, is evinced by its being acted upon by the stimulus of air; and its life, in its solid or coagulated state, is evinced by its not acting on the living solid as dead matter, and from its becoming vascular. Mr. Hunter neither, as I think, supposes coagulation a morbid state, or from his words can it be inferred.

Dr. Hendy, however, intent upon refuting the opinion, makes Mr. Hunter to say so; and also in the next paragraph, what cannot possibly be inferred but from imagination alone. Mr. Hunter, he says, considers the life of the blood in the following sense: "He considers a muscle cut out of the body to be alive, as long as it
" continues capable of being acted upon by
" a stimulus of any kind; that is, as long
" as any part of it retains its irritability." To which he adds, "Mr. Hunter agrees to
" this, by saying, that the muscles of a
" turtle continue alive a great while after
" the animal as one whole is dead." He having then fixed these as data of Mr. Hunter's, and as if at once to refute the possibility of life existing in the blood, proposes the following as a query: "Does the coagu-
E " lum

“ lum possess the least degree of this irrita-
“ bility or life? as Mr. Hunter (he says)
“ is pleased to term it.” To which he
immediately answers: “ No; it has cer-
“ tainly lost its irritability; and therefore
“ (says he) Mr. Hunter himself not only
“ agrees that coagulation is a morbid
“ change, but by his own arguments
“ proves that the coagulum of blood is
“ a dead matter.”

To these arguments, seemingly spacious,
I would only observe, that having atten-
tively considered all of Mr. Hunter's argu-
ments, it was impossible for me to draw
such inferences as Dr. Hendy has done; for
it evidently appears to me that Mr. Hunter
uses the analogy of irritability in muscular
fibre, not with the view of shewing that
the blood possesses a life exactly analogous
thereto, and that from the application of a
stimulus similar consequences will follow;
(this cannot be expected, owing to the evi-
dent difference of organization; by which
we readily account for the different effects
which follow the application of the same
cause,) but merely with the intention of
demonstrating, by a familiar instance, that
blood,

blood, although separated or unconnected with the source of life, may yet, like muscular fibre, retain this life for a length of time, although unconnected with the source of life. Hence we cannot but draw a different conclusion, the data being thus changed, and instead of saying with Dr. Hendy that we must infer, from Mr. Hunter's own arguments, that the coagulum of blood is really dead matter, we say, that the coagulation is a consequence of the previous application of a stimulus to the blood when fluid, which had the effect of producing it without destroying, or even diminishing, the vital principle.

These arguments then, to me, sufficiently prove that the blood, when extravasated, does not act on the animal-living solids as a dead, but living matter; consequently were we in this place to enquire what its effects were when circulating in its proper vessels, we should be irresistably led to the conclusion, that these effects must be a consequence of its living power. This and the former are farther proved by arguments to follow under another head, and which demonstrate directly the living power of the

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blood.

blood. Waving farther enquiry then, as to this head, we proceed to the second, and endeavour to prove that blood is capable of being acted upon by a stimulus.

Sensibility to a stimulus is only the property of living bodies ; consequently, if we find the blood possessed of this power, we cannot but conclude it alive. Mr. Hunter, sensible of this, advances the following, and what, I think, true proposition : “ Blood,” says he, “ is capable of being “ acted upon by a stimulus, for it coagu- “ lates on exposure, as certainly as the ca- “ vity of the thorax or abdomen inflame “ from the same cause.” As I have already hinted the truth of this position, and the necessity of its being admitted, a slight digression, if it should occur, will, perhaps, be readily pardoned. Dr. Hendy* ; however, thinks, with some degree of confidence, that he refutes it by merely mentioning, in the most sceptical manner— “ Any person would be laughed at, were “ he to affirm that a jelly of any kind “ was alive, because in a certain heat it

* Ess. Gl. Sec. p. 67.

“ continues fluid, and on a diminution of
 “ this heat, or on exposure to cold and
 “ air, it coagulates.”

Had I not been possessed of, perhaps, a more than common power over my risible muscles, I certainly, upon reading this, could not have refrained from laughing, to find a physician of Hendy's evident erudition treat in such a manner so evident a forcible argument ; but certainly ignorance of this last could only have led him to do so, unless, destitute of an argument to invalidate it, he had, as a dernier ressource, recourse to turning it into ridicule : when such arguments as these are used, it is a sure proof to me of the want of others more forcible ; for by this argument, when seriously considered, it can be shewn that there exists in the blood a principle which can be nothing but living, which is as easily acted upon by air, and peculiar consequences result therefrom, as result from the action of stimuli, either mechanical or chemical, on parts possessed of irritability, although detached from the animal.

Besides, had Dr. Hendy seriously considered the matter, he would never have
 drawn

drawn any analogy betwixt the coagulation of blood and that of jellies, it being evident to every observer, that the coagulation of the latter depends upon a diminution of its heat, and by an increase of this heat can again be rendered fluid, while that of the former depends upon no such cause; and when once coagulated, cannot easily again be rendered fluid. It also evidently coagulates in some parts of the system, when its heat is as great as when circulated in its proper vessels.

But to entirely subvert this futile argument of Dr. Hendy's, (nothing less than futile can I call it), we need only to have recourse to those experiments of Mr. Hewson's on the blood, which brought to light many important facts; there it is pointed out in the clearest manner, that the coagulation of blood does not depend either upon a diminution of its heat, or want of motion, but upon the action of air, although when applied it be as warm as the blood itself*.

* Hewson's Experimental Enquiry, Exper. 6. p. 20.

This certainly, and without doubt, destroys the analogical argument of Dr. Hendy; for although we expose jellies of any denomination to the action of air alone, yet if we do not diminish that heat on which their fluidity depends, they will not coagulate. Hence it is evident their coagulation depends upon a diminution of that heat to which they owe their fluidity, while the coagulation of blood depends upon the action of air on its vital principle, and not upon a diminution of its heat.

To shew which in a still clearer point of view, if need be, recourse again to Mr. Hewson's Experiments will sufficiently demonstrate that cold, when applied to blood detached from the system, but included in its proper vessel, so as to render the access of air inadmissible, has, when continued for a length of time, although many degrees above freezing, the power of lessening considerably the disposition of that blood to coagulate, instead of coagulating it, which ought to occur were Hendy's analogy just; and what confirms this the more is, that blood can be even froze without coagulation taking place, and even after being thawed,

thawed, by the action of air coagulated; and finally we find, by Exp. 26th of Hewson, that blood included in its proper vessel, and immersed in water at 38 deg. above (O), and suffered to remain there for nearly twenty-four hours, upon taking it out and opening the vessel the blood was found to be fluid, though viscid; and although it was placed upon the window of a moderately warm room, and examined carefully from time to time, it never had any appearance of coagulation; on the contrary, it remained fluid till it was dried by the evaporation of the water. This, without the least room for doubt, demonstrates that the coagulation of blood does not, as jellies do, depend upon a diminution of that heat, obvious when in a fluid state.

In addition to these arguments, Dr. Adye *, with a view to controvert the opinion, adduces the following: “ Quod
 “ multæ res inanimatæ eodem modo affi-
 “ ciuntur; albumen, exempli gratia, ovi,
 “ quod nemo vivum esse existimabit.”

* Diff. Inag. de Sang. Circuitu. p. 88.

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This argument of his, even supposing the albumen dead matter, I would object to, and say the analogy was not just. Will the albumen coagulate upon mere exposure to air? No, it will not; then, as we have shewn this the power which coagulates blood soonest, and has no effect on the albumen ovi, the analogy does not hold good, although both substances coagulate upon an increase of their heat; and even then I cannot suppose that this occurs before all the life which both substances possess is destroyed, provided the external air has not access to the blood.

And to conclude, Dr. Adye advances that he cannot suppose it alive, or capable of being acted upon by a stimulus, because it neither shews any signs thereof when the more acrid stimuli are applied; nor does alternate relaxations and contractions take place, which is what we see happen when the solids are acted upon by stimuli, either mechanical or chemical.

To these arguments I would object, and say, that although from the action of stimuli, on the irritability of muscular fibre, relaxation and contraction alternate,

yet it does not follow that the same effects should take place when stimuli are applied to the blood ; the difference of effect arising not from any difference in the life, but entirely from the different organization of the muscular fibre ; for to this last is entirely owing the phenomena of irritability, that is, a certain organization conjoined with life ; which being the case, we cannot reasonably suppose, the organization being wanting in the blood, either absolutely, or as to kind, that the same phenomena could occur from the application of the same stimulus.

The same arguments also sufficiently refute what Dr. Adye infers from the different action of common salt on the muscular fibre and the blood ; for evidently when applied to the blood, as singular consequences follow, as when applied to the irritability of a muscle ; and to conclude this part, we might as well say that a muscle did not possess life, because we see that certain powers, when applied in a great degree, have the property immediately, and without any obvious effect, of entirely destroying the irritability or life of the fibre, so that it can be no longer acted upon

upon by any stimulus; as that the blood has no life, because common salt, and many others when applied thereto, have the property of rendering it, when undiluted, incapable of being acted upon by the stimulus of exposure.

Having thus, I think, clearly established Mr. Hunter's position, that there is a principle in the blood, by the action of air upon which the coagulation thereof depends, and satisfactorily invalidated all those arguments which have been brought against it, it will nevertheless still tend to render it more unexceptionable if we shew that it acknowledges the same laws as other living bodies, and that at different times it is variously affected from the different state of life at the time, which leads us to the consideration of another of Mr. Hunter's arguments, which is, "that the more it is
 " alive, that is, the more the animal is in
 " health, the sooner it coagulates on ex-
 " posure; and the more it has lost of its
 " living principle, as in the case of vio-
 " lent inflammation, the less it is sensible
 " to the stimulus produced from its being
 " exposed, and the later it coagulates."

The verity of this observation, as to fact, is sufficiently evident, and the inference therefrom is also, perhaps, just : yet from my observations, in another part, I am led to conclude, that, perhaps, the real life of the blood, in those persons labouring under inflammation, is not diminished, but only from the violent action of the cause inducing inflammation, or a different state of action of those organs which give to the blood its animation, it is rendered almost insensible to that stimulus which formerly freely acted upon it. This argument has also analogy for its support ; for we often see that, by the long-continued application of a stimulus to a muscular fibre, it becomes gradually less sensible thereto ; yet notwithstanding this, its real life is not diminished.

Having then, as I think, necessarily explained thus much, we will next consider those arguments which have been advanced against this position ; and first, those of Dr. Hendy, who at once objects to it in whole, by saying, “ that from this it would appear to be a mere passive kind of life
“ that the blood enjoys, for it is totally
“ depen-

“ dependent on the animal powers which
 “ cause it to circulate.”

But this is certainly no argument ; we might as well say the solids were not alive, because they were dependent for this life upon the blood ; for certainly their life is as intimately connected with the blood, as the life of the blood is connected with the solids : and in another place I will attempt to shew, that both life and sensation are, perhaps, more commonly affected through the action of different substances on the life of the blood than has been commonly supposed ; but, adds he, “ it must be a
 “ curious kind of life it possesses, as the
 “ more it is alive, the easier it is affected
 “ by a stimulus ; now it is the reverse in
 “ every other kind of life ; for,” says he,
 “ the weaker the principle of life is, the
 “ more violently it is affected by slight
 “ stimuli.”

This may certainly be the case ; yet it is no argument that life does not exist in the blood ; for those effects which follow the application of stimuli to muscular fibre, when its principle of life is weakened, is, perhaps, more owing to the state of organization

zation than that of life ; for as in the blood life exists in its most simple form, and unconnected, at least, with that organization which constitutes muscular fibre, from the state of life at the time being alike in both, supposing them so, yet from the want or difference of organization we thus easily account for the different phenomena which result from the application of the same cause to both.

But viewing the subject as I have already done, and considering that in inflammation the life of the blood is not diminished, only from some cause rendered more insensible to the application of a stimulus, a ready solution at once is afforded of Dr. Hendy's argument, without having recourse to any other.

But in addition to these arguments of Dr. Hendy, Dr. Adye endeavours to combat the idea by arguments drawn from three sources ; but the two first of which evidently rest on so slippery a foundation as not to need a serious consideration ; for we have already proved, and it is, indeed, evident, no analogy can be drawn betwixt the effects which follow the application of the same

same cause, to the blood and living muscular fibre : the arguments, however, are drawn from this analogy, which, being false, are at once refuted : and as to the third, which we propose to consider at more length, I expect it will share the same fate ; it is :

“ Si sanguis serius coiens suæ a sanitate
 “ aberrationi est referendus, unde citius
 “ cogitur in morbis putridis, quam in in-
 “ flammatoriis ; cum in illis, quam in his,
 “ fluida majis afficiantur. Præterea cum
 “ contagio, si ab effectibus quos edat judi-
 “ care liceat, vim magnopere sedantem
 “ habiat, in morbis ab hac originem dicen-
 “ tibus minus irritabilis esse debet sanguis,
 “ ideoque serius in coagula abire.”

In the first part of this argument Dr. Adye asks why, if the late coagulation of the blood depends on a lessening of its vital principle, is it sooner coagulated in putrid than in inflammatory diseases, as in this latter the fluids are less affected than in the former.

To this I would answer, that although the late coagulation of the blood, in one instance, depends on a diminution of its living power, we cannot infer it to be a
 general

general rule, and occurring in every case ; for although we find the blood coagulates later than usual, in both putrid and inflammatory diseases, we cannot thence conclude that in both the living power of the blood is diminished ; on the contrary, as I have already hinted, and will hereafter endeavour to prove, every circumstance tends to shew, that in inflammatory disorders the vital principal is nothing diminished, only from violent action, (which Mr. Hewson has shewn is a cause adequate to explain the phenomena) rendered not so sensible to the application of a stimulus ; while in putrid disorders every circumstance connected both with their history and treatment, sufficiently evince the diminution of life in every function ; and, without doubt, a real diminution here occurs of the blood's life, owing to which it coagulates later than usual.

This view of the matter is even farther confirmed from another circumstance, and that only a diminution less or more of the blood's living principle occurs in putrid disorders, is its coagulating in some cases sooner than in the inflammatory ; while in
other

other cases of putrid disorders the diminution of the blood's life is so great, that no coagulation even follows from exposure.

From all this, then, the evident answer to Dr. Adye's question is, that the lessened disposition of the blood to coagulate in inflammatory disorders, depends on an insensibility of its life to the action of a stimulus, solely induced by violent action, and without any real diminution of its life; while in the putrid, although the blood appears more affected, and is in reality so, yet its lessened disposition to coagulate depends upon a diminution alone of its living power, and from a greater or less diminution of life or vigour the stimulus of air produces its effects with more or less facility.

To the second part of the argument, that which suggests a doubt about the action of contagion, I have only in this place to observe, that it is more than probable that, in such cases, contagion has a sedative power; but what is more uncertain, is the ascertaining on what this sedative power acts; to me it appears more than probable,

and I will state my reasons for this hereafter; that its first action is on the blood, and that the disordered state of sensation, and the power of motion, are only consequences resulting from the action of blood thus changed by contagion; and hence we really find, that blood in these disorders shews that its life is diminished: and therefore in answer to the question, we say, and observation seems to confirm it, that in proportion to the virulence of the contagion, and consequent diminution of the living principle, the blood in these disorders has more or less tendency to coagulation. And this answer remains the same, whether we suppose the blood primarily affected, or that the disordered state of it is only a consequence of the morbid alteration in the action of the moving fibre, or nervous system induced by the specific stimulus of each contagion, and by that alone affecting our fluids.

Having thus then, as briefly as the nature of the subject would allow, considered those effects which follow the application of a stimulus to the living principle of the blood, and shewn that it acts thereon in a peculiar manner, but agreeable to those laws

laws which govern living bodies, and having also seemingly invalidated those arguments which have been brought against Mr. Hunter's data, we, without farther reasoning, proceed to the consideration of the third section of the definition, which is, that it is possessed of a power by which it resists the action of those causes which tend to its destruction.

As we have said before, this is a constant and invariable effect which results from the action of certain agents, on a living principle ; living bodies are alone possessed of it ; and hence if we find the blood possessed of such a power, we must irresistibly infer a living power in it, although we were destitute of any other argument in support of it. Mr. Hunter, sensible of this, brings forward the following argument, founded on fact, and strictly just : he says, “ Blood taken from the arm, in
“ the most intense cold which the hu-
“ man body can bear, raises the thermo-
“ meter to the same height as blood taken
“ in the most sultry heat. This (adds he)
“ is a strong proof of the blood's being
“ alive, as living bodies alone have the

“ power of resisting great degrees both of
“ heat and cold, and of maintaining, in
“ almost every situation while in health,
“ that temperature which we distinguish
“ by the name of animal heat.”

The opponents of the opinion, that life exists in the blood, allow, sensible they cannot do otherwise, that this argument is a strong proof that life can resist the action of external cold; but they will not admit that it is any proof this power exists in the blood; on the contrary, with a view to its refutation, Dr. Hendy asserts the following: “ That blood, drawn in several different circumstances of the air, in less
“ than two hours will be cooled to the
“ degree of the surrounding atmosphere.” That this is the case from experiment I am fully convinced; but from the same source also I am as fully convinced, that a quantity of starch dissolved in water, so as to make a fluid as thick as blood, and being of the same temperature as the blood drawn from its vessels, in like quantity, and placed under similar circumstances, comes to the temperature of the air considerably sooner than the blood. From his experiment,

however, he infers, “ that blood, independent of the animal system, cannot retain heat ; hence it cannot be alive.”

To this conclusion I only remark, that I cannot suppose any one could think it possible that blood, unconnected with the source of life, could resist the action of cold, or retain its heat, for a longer time than till that quantity of life attached to it was destroyed, which takes place sooner or later, according to the quantity of life, or the force with which the hurtful agents act. But that it can resist the action either of heat or cold, and that too for a considerable time, even although they be applied in no low degree, I am fully convinced from similar experiments as those instituted on the egg. But as was formerly hinted, these experiments being made upon a small scale, I shall, instead of relating them at full length, merely mention the result.

Having immersed a quantity of blood just drawn from the arm of a sound person, in a freezing mixture, till it was frozen, and after having continued in that state for some time, I thawed it, and brought it to the same temperature as when drawn from
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the vein; I then immersed it again into the same freezing mixture along with another quantity of the same blood directly drawn from the same person, and in exact proportion; but the one which had been frozen and thawed again, froze many minutes sooner than that directly from the vein. The result of this experiment was as I expected; but I should have diversified it considerably, had not want of materials prevented me, which I much regret. At the same time I repeated the same experiment on blood contained in its proper vessel by ligatures, and found the result to correspond, taking into account the little quantity of blood subject to the action of the cold.

I also found by subjecting a quantity of blood to the action of electricity from a large battery, that its power of resisting the cold was considerably diminished; and in one case I thought its disposition to coagulation much diminished, nay entirely destroyed.

These experiments, imperfect as they are, suggest many others, and which the first opportunity for procuring materials

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I will perform, especially if I can procure any of the American poisons, or viper's venom, which, I think, possesses the power of immediately destroying the blood's life, and which by a freezing process I shall be able to ascertain.

From those experiments it would appear then, that blood, unconnected with the source of life, has a power of resisting, as other living bodies have, the action of cold as long as this life remains, which fully confirms Mr. Hunter's argument; but without inferring any thing from these experiments, Dr. Hendy's objections rest on so slippery a foundation, that they can be invalidated by merely turning the argument against himself.

He supposes life can only be attached to the solids of animals; but this, taking his rule of life along with us, we could not infer, because we find that a muscle cut out of the body, and unconnected with the source of life, does not resist the action of cold, or retain its heat, for a longer time than blood does; it is evident, however, both are alive, and both resist the action of these hurtful bodies so long as they possess
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this life ; but being unconnected with the source of life, this power of resistance cannot continue for any considerable length of time.

Thus, then, Mr. Hunter's argument remains in full force—a strong proof of the blood's vitality.

After which we proceed to the consideration of another argument of Mr. Hunter's—the last and only one which we have not considered. A doubt, however, may arise, whether it more properly belongs to this head, or to that which follows ; but for sufficiently obvious reasons we consider it under this ; it is, “ blood preserves life in different
“ parts of the body. When the nerves
“ going to a part are tied or cut, the part
“ becomes paralytic, and loses all power of
“ motion, but it does not mortify ; but if
“ the artery be cut, the part dies, and mortification ensues.”

Thus, then, it is that blood flowing into a part, although unconnected with the nervous system, resists the action of heat and moisture, and consequently putrefaction, by which life is continued. Viewing it, then, in this light, and that it is from being
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ing possessed of a power of resistance to those agents that life is preserved in such situations, it naturally falls to be considered in this place.

The authenticity of this, as to fact, is sufficiently evinced by every day's observation; yet the induction from the data is endeavoured by Dr. Hendy to be combated; for he only allows this "a proof that blood is essentially necessary to the life of a part; and, if proving any thing, it is, that nourishment is chiefly conveyed by the arteries, and not by the nerves." Even in this view he thinks it hardly satisfactory; "for although (says he) the larger branches of the nerves were cut or tied, yet the almost infinite division of nerves would lead us to conclude, that a number of these small branches were still left;" and concludes the whole by affirming, "that though a part deprived of blood mortifies, yet the fluid which sustains it may not be alive itself; for (says he) the whole animal body cannot exist long, independent of certain ingesta taken into the stomach;" and adds, "because this is highly necessary to life, we cannot from
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“ thence infer that the ingesta must have
“ life.”

To these arguments, highly ingenious, and, at first sight, not a little conclusive, I have only to remark, that were not the blood endowed with a living principle as it circulates in its vessels, it could not, independent of the nerves, preserve life—I say independent; for were these small branches, (and this is only a mere supposition) which Dr. Hendy supposes to remain uncut, capable of preserving the life of the part in as high a state as before, which experiment has proved to be the case, although deprived of sensation, nature has certainly wasted a great deal of nervous energy to no purpose. None of Nature’s works are, however, in vain; for sensation, and the purposes thereof, she has alone employed them; and upon their destruction sensation ceases.

Moreover, that nervous influence is the only source of life, is irreconcilable with many of the phenomena of nature: for example; it is found by repeated experiments, that upon dividing high up the whole nerves of the lower extremity of a

frog, and leaving the arteries uncut, that life can be sustained in that limb, for almost any length of time, (without sensation) as perfect, and the parts as plump, as when the nerves remained uncut.

To shew that this is inexplicable by any other means than supposing the blood possessed of a living power, will, I think, be no difficult task. We have only to mention, that from many arguments and some experiments, it is demonstrated, and, I believe, generally admitted, that the solids of our body, as well as the fluids, are always changing; that is, in the former there takes place a constant absorption from, and as constant a deposition to them by the arteries. This deposition, physiologists of our days generally allow, is made by those means we have mentioned, directly from the blood, and what is absorbed is as evidently poured into the blood: only apply this to the case in point, and, beyond a doubt, it demonstrates the living power of the blood; for from it we cannot but infer, that in no long time, what constitute, when the nerve was cut, the solids of the limb, particularly the muscular and other soft

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parts,

parts, would be entirely absorbed; and what new matter was deposited, which we see was in proper quantity, from the plumpness of the limb, ought to have been dead matter, since the nerves were cut, unless we suppose that the blood was alive; which, were it not, mortification, according to this view of the subject, would soon have followed, and as certainly as if the circulation in the arteries had been stopped.

Even supposing some small branches left (for the sake of argument) uncut, yet it is not possible to conceive them adequate to animate a quantity of matter so great as to require those large trunks which we see sent to the limb. We even here suppose the nerves give to the muscular fibre that life which it possesses; yet in another place I will attempt to shew, that simple life, if I may be allowed the expression, exists independent of nerves; and that the muscular fibre possesses such a life, and that too immediately derived from the blood, conjoined only with a peculiar organization, is what I expect to make not a little probable.

Thus,

Thus, then, have we invalidated those arguments which have been brought against the first part of our position, and made it, I think, sufficiently evident that blood is not only necessary to the life of a part, but that it is even capable of sustaining life in a part unconnected with the nerves; and that this could only take place, we have made evident, from its being possessed of a living power.

Another argument which naturally occurs here, and which seems to render the idea of life in the blood still more probable, if need be, can also be drawn from this waste and supply of our solids.

What is taken in by the absorbent vessels, (but how this is affected I cannot even pretend to say) evidently before that, was possessed of life. Now does its being absorbed destroy this life? or can absorbent vessels not take in living matter? But, perhaps, allowing that they can, yet it may be asserted, that they only take in those particles of the solids when dead, and thus render superfluous the other two questions in one point of view.

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To me it appears, however, that were the particles of our solids only absorbed when dead, and verging to putridity, all those effects would follow their admission, in proportion to their quantity, as follow the absorption of evidently dead and putrid particles, as in cases of gangrene, and disease would always, in a certain degree, be present. Besides, it is impossible to figure to ourselves how dead solids could exist, without shewing their effects as such ; and as impossible to suppose how one particle is deprived of its life, while the rest possess theirs. But what end absorption from our solids, and a constant deposition thereto, serves, I will not take upon me to say ; certain I think it is, however, that they are in a living state when absorbed, and thus we have an evident source of vitality in our fluids, unless it can be shewn lymphatic vessels, or vessels in general, have a power of destroying this life ; but so far from this, I trust, I shall make it appear they in general possess a power of animating the blood.

Thus, then, are we led to the consideration of the last part of Dr. Hendy's argument,

ment, which is, “ that because certain ingesta taken into the stomach are highly necessary to life, we cannot infer that the ingesta must have life.”

To this argument, in this place, we will merely content ourselves with observing, that it is evident our animated nature derives its origin from inanimate matter : and this question leads to the enquiry, where or in what place matter is so changed, so as, besides being governed by the laws of dead bodies, it acknowledges that of the living. In another place I professedly treat of this subject, and therefore wave the enquiry. I may only observe, that from two circumstances it is evident that the matter taken into the stomach as ingesta, has, before it enters the red vessels, its property so changed, that it does not produce the effects of dead matter ; otherwise we should find that milk, a substance extremely analogous to chyle, mild and bland, would shew no bad effects when immediately injected into our vessels : but this is far from being the case.

The other argument is, the peculiarity of the process of digestion, and the well-known

known wonderful power of the action of vessels.

From this it is evident, then, that I even view the matter taken into our system as animated to a certain degree before it enters it; but this, it is evident, we are not under the necessity of proving to invalidate Dr. Hendy's argument. We have only to say, and, indeed, it is so, that it is a matter of no consequence to us at present what the effects of the ingesta are, and where the ingesta are animated, since we have, by a multiplicity of argument, and even by the argument to which he opposes this reasoning, demonstrated a living power in the blood.

Having thus, then, considered the three first sections of our definition, and, in my opinion, confirmed them from experiments and observation, we are naturally led to a consideration of the fourth and last, which is, that the living principle of the blood is the cause of all the actions which it induces. This, it is evident, will be a task not a little difficult, as it is involved in much obscurity; it would lead me to investigate many hypotheses deserving attention, either from their
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own plausibility, or the high repute of their authors.

This, however, is inconsistent with the nature of this essay ; besides, it is a task to which I am not equal ; I must, therefore, content myself with merely mentioning the defects of some of the late, and, I may say, generally received, systems, leaving the greater number to fall by their own insufficiency, or the arguments of others.

Having, then, already demonstrated that blood, when extravasated, is governed by the laws of living matter, we certainly cannot with justice infer, that those actions which it induces, when flowing in its proper vessels, result from a cause of which by a principle it is divested. Thus it is evident that the opinion, though novel and new, is not unreasonable ; on the contrary, a natural and direct induction from its animated nature.

That the blood, then, in our system being living, its effects cannot be considered as resulting either from its being passive, or as dead matter, I will attempt to shew, that by viewing it in such a light, that is, as dead matter, we cannot explain certain functions of the animal body ; while a ready and

even direct solution is the consequence, when we view it possessed of life, which it evidently is.

To do this, then, and prove my assertions, it is necessary to bring forward those effects which the blood shews on the living solid when extravasated. These, in a former part, we have shewn, not to be the same as those which result from the application of dead matter, and that no effects are produced but such as result from the action of one living power upon another.

We have also demonstrated, that the blood is possessed of two other characteristics peculiar to a living state ; a power of resistance, and a principle of sensibility. Hence it is not unreasonable to infer, that when circulating in our system it is possessed of a living power : therefore the only natural and direct induction from these *data* is, that those effects which result from its action on the living solids, independent of quantity and force, must be a consequence of this principle acting thereon ; for being deprived of a constant property of dead matter, we cannot suppose those effects
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which result a consequence of a power of which it is not possessed.

From these arguments alone, then, our position appears to me fixed and determined, and no farther proof wanted to the establishing thus our fourth and last section of the definition: but we can go farther, and, in my opinion, demonstrate the impossibility of admitting the blood to be dead, and that it acts chemically on our solids—a strong proof, independent of any other, of the blood's vitality.

To prove this, then, we will briefly consider, at this time, the theory of circulation alone; to explain which many different hypotheses have been invented. Doctors White, and Monro, junior, however, after invalidating all other hypotheses, propose their own, and which, they think, rests on a sure foundation; and which at present, as far as I know, is the theory universally admitted.

From what I have been able to collect from their works, it appears to me that the doctrine which they inculcate is, that from the irritability of the heart and vessels, (it does not become me here to enquire from

whence this irritability is derived) acted upon by the blood chemically, that is, from it, stimulant, saline property, conjoined with a certain degree of distention, result the action of the heart, and the whole phenomena of circulation.

That a certain state of the heart and vessels is necessary to circulation, and which may be termed their irritability, is what must be granted; but that this state is acted upon by the blood chemically, that is, as we see other stimulants act on the same power, is that part of the doctrine which, I think, cannot be granted; for were its action such, being dead, (for as such they consider the blood) the same phenomena would result which follow the action of other dead matter. The phenomena, however, are different; therefore we cannot but infer a different cause.

Let us only consider what the consequences are of the application of any one stimulus to parts endowed with irritability. The effects are too well known to require much explanation, as every observer is sufficiently informed, that after a stimulus has been used for a length of time, if we wish

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to induce the same effects which it induced at first, we must increase the dose in proportion to the length of time, or the frequency with which the stimulus has been used.

Hence we must, agreeable to this constant and universal law respecting the action of dead matter on muscular fibres, infer, that were it from a chemical stimulant quality existing in the blood that the action of the heart and vessels was induced, this stimulant quality ought always to be on the increase, to enable it to produce its former effects. This, however, does not take place, if we may judge from its sensible qualities, the only test we know ; nor is it possible to suppose it could take place, as we neither know the source from which it could be derived, nor the mechanism by which it could thus be uniformly supported.

This reasoning applies equally to the function of secretion, and all others in which blood is actively concerned.

The only inference, then, which now appears to me can be drawn from those *data* which we have thus established (for-

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mer inductions being obviously erroneous) is, that those actions excited in a healthy state by the blood, if not by quantity or force, are a consequence of its vital power, supposing one living power acting on another, perhaps something analogous to the stimulus of the will acting on the principle of motion.

Having thus, then, fixed that those natural actions which the blood induces, when independent of force and quantity, are a consequence of its living power, it would naturally from thence appear, that from a destruction or diminution of this power a morbid state of action would be induced.

That this is the case, and that we shall be thus enabled to explain the actions of many substances, not hitherto ascertained, is what I will endeavour to render probable; previous to which, however, I shall endeavour to remove some objections which I conceive may be advanced against the opinion offered: they are seemingly strong, and at first sight not a little conclusive; but that they are more imaginary than real, observation, I trust, will demonstrate.

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It may be alledged, that the fæces, urine, and other excreta, which no one can even suppose possessed of life, nor have they qualities much more changeable, or any of their stimulant properties more increased by age than the blood, yet we find that through life, from their action on the moving fibre, the actions which they induce for their own expulsion, are nearly the same, and perfectly regular at all ages, provided the power acted upon, and the power which acts thereon, are not different from the healthy state.

Here, then, we have to appearance a strong proof in support of the generally-received opinion ; yet to me it appears, that observation sufficiently demonstrates that those actions which are induced, by either urine or fæces, for their own expulsion, result not from any quality of these excreta in a natural state, but from quantity alone ; for were it quality, we certainly would be constantly discharging both urine and fæces.

That quality, however, will have effect, is what we daily observe in cases where we wish to induce catharsis or diuresis : yet
this

this same quality, if continued for a length of time, loses its effects also, so that no increased action results from it, unless its dose be gradually augmented. This, then, is almost equal to a demonstration that those fluids poured into the stomach, intestines, and bladder, although to the taste evidently stimulant, yet from custom this stimulant quality has no effect on the irritability of the intestines, &c. Increased discharge may also occur, independent of the stimulant quality of the excreta, from the disposition to action being increased, owing to which state actions are induced the more or less easily.

Thus it would appear, then, that the stimulus of distention alone is that cause which induces the action, by which excretion is affected in a natural or healthy state of the body.

To this, however, it becomes us to remove another objection, to appearance not a little strong, and which seems to demonstrate that a certain quality of the fæces, as well as quantity, is necessary for the discharge in a healthy state. For it is found that in jaundice obstipatio occurs; and this
is

is explained from the want of the usual stimulus of bile in the alimentary canal. Here, however, I beg to differ from those who give this explanation, which, I believe, is universally received, for those reasons already given concerning the effects of custom, and that explanation which appears to be the natural, is, that the obstipatio is owing to the lessened disposition of the moving fibre to contract.

This lessened or diminished irritability is evident over the whole system; it seems to be owing to the presence of bile in the circulating system either acting immediately on the principle of motion, or through the medium of its action on the blood; and, perhaps, the diminished state of motion in the intestines may be somewhat owing to the less perfect state of circulation in the system of the vena portarum. That other excretions are increased, appears to me no argument against the diminution of irritability, since it is evident these may be owing to the increase of stimulant power. This is evidently the case in that by urine; for although the fibre be less irritable, yet from bile a new and strong stimulus is given sufficient

ficient to counterbalance the lessened disposition of the moving fibre to contract.

Having thus, then, invalidated all those arguments which appear to me at present can be brought against the proposed theory of the blood's circulation, and seemingly proved that all the actions which the blood induce, are a consequence of its principle of life, we next come to say a few words concerning morbid actions, induced from a morbid state of the blood.

That such cases occur we have sufficient experiments to confirm, particularly those instituted by the learned and indefatigable experimenter, the Abbe Fontana, in his Treatise on Poisons, who appears highly sensible some principle exists in the blood with which we are but little acquainted: "but," says he*, (speaking of the action of the venom of the viper on a principle in the blood,) "what this new principle is, and what the organs that secrete and convey it to the vessels—in this," says he, "experiment could alone furnish me with some light, and conduct me to

* Vol. I. p. 329.

"some

“some new truth. But,” concludes he,
 “where are the experiments to begin?”

By experiment I think I have demonstrated a principle in the blood, which is that which produces all its effects, if independent of quantity and force; and by induction then, together with experiment, I will attempt to demonstrate, that it is on this principle the poison acts: but were we to enter into into a full discussion of this subject, it would far exceed the bounds of this essay; for connected with it is the discussion of many intricate and obscure pathological subjects. Perhaps the humoral pathology so long discarded may yet, for several reasons hereafter inferred, be revived. In saying so, I do not mean to infer that any of the antients' theories concerning the particular diseases existing in the humours, will be found just; on the contrary, all I wish to say is, (and we may as well deny the existence of the fluids as deny it) that there exists, in not a few cases, a morbid state of the blood, which, when present, is always a cause of disease, and not unfrequently of death itself.

That this morbid state of the blood is in many cases the primary disease, is what I expect to make not a little probable; and also that a morbid state of action in the solids, will induce, as a consequence, a morbid state of the blood.

That the fluids are often in a diseased state, or, at least, a state different from that in health, is manifest even from their obvious sensible qualities; but that a diseased state thereof may exist without any obvious change in these qualities, we may necessarily infer from our ignorance of what the principle of life consists; yet in such cases it will commonly be in our power to shew its morbid state, from its being no longer governed by those laws which govern blood, evidently endowed with a principle of life, as we have demonstrated that air acting upon this principle of life by its stimulus coagulates the blood. Hence the natural conclusion from this is, that when blood coagulates with difficulty, or not at all, when exposed to the air, that its principle of life is in what may be called a morbid state; and it is as evident this morbid state may

may either consist in an insensibility of this principle, (unconnected with diminution) to those stimuli which formerly acted with facility upon it, on a partial diminution of this principle, or a total diminution of the same.

That the first state, insensibility of the blood to a stimulus (unconnected with diminution) may occur, the analogy of other parts acted upon by stimuli render sufficiently probable. This state, when it happens, however, I view merely as secondary, and the consequence of a morbid action, perhaps of those organs which contribute to the blood its living power; and that even from a different state of action of these organs, induced by different exciting powers, result the different or various degrees of insensibility, observed in the blood in different species of inflammatory diseases, where, from several experiments made by freezing, and similar to those already related, I was led to infer, that only insensibility to stimulus, without diminution, occurred, while diminution by the same experiments was evident in the putrid.

In

In those cases in practice our chief attention ought to be directed to counteract the morbid state of action; for, on a removal thereof, in no long time, the effects commonly cease, there being a cessation of the cause.

That the second state, a diminution of the principle of life in the blood, may occur from causes acting immediately so as to produce this diminution, we are bound to believe, from many observations, and more particularly from this general law, that certain matters which in a given degree can produce the annihilation of life; yet from a deficiency of this quantity, are only adequate to produce a diminution of this life, and which diminution will always be in proportion to the quantity or force with which the hurtful power acts. Hence, perhaps, the different degrees of putrid diseases in which the blood is differently affected.

But a question naturally occurs here. Does the cause inducing these diseases act primarily on the blood? and do the symptoms and consequences which follow, derive their origin from this source?

That

That this is the case, recent experiments, and inductions therefrom, appear to me to render it probable. Many substances, as the poison of the viper, ticunas, &c., which were formerly thought to act on the nerves, experiment, our only sure guide, has proved to be false, and that they exert their virulence on the blood alone: and hence, although contagion has not been made the subject of experiment, as it is out of our power, yet, from many circumstances attending the history of affections induced by this cause, we have great reason to believe that its primary action is on the blood, through which the principles of sensation and motion are affected; for on the healthy state of the blood, we have shewn, depends the healthy state of these functions. Circumstances also attending the cure of affections induced by this cause, seem to render the opinion not a little probable, all of them generally consisting in such means as tend to increase action and supply the want, while even some of them, opium, and from analogy may we not suppose the rest, Fontana has shewn to act immediately on the blood.

Here, however, from the nature of the thing, will venture to suggest a practice, although it has the appearance of novelty. In many cases of putrid diseases, as the plague, fevers of the typhoid type, scurvy, &c., I would recommend transfusion, or the abstraction of a certain degree of the diseased blood, and the insertion of a quantity of healthy from a person in a sound state. The only objection, or rather hazard, in this practice, is, our ignorance of the proper quantity which can be inserted; there being, I think, from many other morbid cases, sufficient reason for believing that there exists a certain balance betwixt the powers to be acted upon, and those which act thereon; the powers then, in these cases, are evidently lessened, consequently those substances which act on these powers must be applied in a less degree, otherwise from their action there will result such an expence of animal powers as would ultimately terminate fatally.

This argument, however, strong as it is, is of no weight in another morbid case,
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depending, as I think, upon a diminution of the vital power, by the retention of that hurtful matter generated by the system itself, and thrown off by respiration; which being suspended from any cause whatever, acts so upon the principle which stimulates the heart, and which I have shewn to be life, that it is no longer capable of acting upon the principle of motion in the heart, so as to induce its contraction and consequent propulsion of the blood through the whole system, on which active life immediately depends. Here, then, the powers are in an ordinary and healthy state; consequently all that is wanted is, that the power which stimulates ought to be rendered healthy also, which could be easily done by transfusion, were we not deprived of the means of withdrawing the diseased blood from the system, from the want of action of the vessels. Hence we are under the necessity of increasing the disposition to action of these vessels by external heat, and, as far as possible, promoting respiration, and the consequent subtraction of the virulent matter from the mass of blood.

Dr. Goodwin, in his excellent essay on suspended animation, has clearly pointed it out, that the cessation of circulation depends on a want of sufficient stimulus in the blood, without venturing to suggest what this stimulant power is which is rendered inert; elsewhere I have shewn, that the only power, independent of quantity, which stimulates in the blood, is its life; consequently if there be a defect of stimulant quality, which there evidently is, the principle of life therein must be affected.

Dr. Goodwin is farther of opinion, that the principle which changes the colour of the blood black, is also the same principle which destroys circulation; but with diffidence I would say, reasoning from analogy, that the principle which changes the blood black, is not the same principle which destroys circulation, or, in other words, destroys the life of the blood; for by experiment it is found*, that the poison of the viper, which evidently, when used in proper quantity, entirely destroys

Font. on Poisons, vol. I., p. 384.

the life of the blood, so far as to destroy animal life, has the faculty of changing the colour of the blood, both of warm and cold animals, black ; yet it is only a poison to the former, being innocent to the latter.

Having thus, then, in a cursory manner, considered a few of those cases depending, either on an insensibility to stimulus, without diminution, or on so far a diminution of the blood's life as that a cure can be brought about by indirect means, we in the last place come to consider that state either depending on an annihilation, or so far destruction of the blood's life, that by any secondary means we are unable to restore life to the animal.

The ingenious Fontana has, I think, satisfactorily proved, that the poison of the viper, ticunas, toxicodendron, cherry laurel, and some other vegetable poisons, act solely on the blood ; no doubt some may deny the experiments ; but by candour I am bound to believe them just, till some person of merit will point out their defects, many of them shewing that both sensation and the power of motion are only

secondary affected ; for these poisons, when applied to the nerves, shew no action thereon : and corresponding with this is the effects of ligature above the parts bit, the circulation being stopped, the disease caused by the venom, as its total action is on the blood, should be so too. This accordingly Fontana found to occur in the cases of animals for which he made use of it *, which never could happen were not the blood the part on which it acted.

We also find that parts amputated, immediately after being bit by a viper, notwithstanding yet in a state of palpitation, to our senses, shew no signs of disease, there still subsisting the degree of heat for a length of time it had before cut out, together with a perfect irritability in its muscles, which continue to move even for whole minutes †.

From these circumstances briefly stated then, we are irresistibly led to the conclusion, that these poisons act upon some power in the blood ; and from the effects

* Fontana, vol. II. p. 48.

† Vol. I. p. 291.

we will endeavour to shew what this power is, having previously made the induction, that although the irritability, which some think is the principle on which these poisons act*, is diminished, and even destroyed, yet this may rather be an effect than a cause, and owing entirely to the change induced in the blood, rather than an effect of the venom on the muscular fibre; for as the sensibility is affected equally with the irritability, and as the venom does not act on the nerves, but on the blood, this diminution of strength and sensation, and likewise the diminution of the irritability itself, may depend on the blood. It remains for us, then, to determine what this power is on which the venom of the viper acts.

Fontana, sensible, as we have already seen, that we must admit the presence of some principle, and that too of a subtle nature, hazards in several places† conjectures, that this principle is secreted by the nerves, and by their termination in the blood vessels poured thereinto, and circu-

* Dr. Milman.

† Vol. I. p. 331, and vol. II. p. 153.

lated along with the blood ; and on the destruction of which principle by these poisons, he supposes it more than probable death ensues.

Elsewhere we speak to this ; but be it as it may, as I have shewn, that the coagulation of the blood, when drawn, depends upon its vital power being acted upon by the stimulus of air ; so when this power to coagulate is entirely destroyed, we must suppose that its living power is destroyed.

That its life in this case is destroyed by the action of these poisons, and not, as in some other cases, only rendered insensible to what was formerly a stimulus to it, both injection of it into the vessels, and simple mixture with blood drawn, sufficiently demonstrate.

In the former case, from the action of the viper's poison injected into the veins, a considerable quantity of the blood is fluid, and deprived of the power of coagulation, with an evident tendency to putrefaction, while another part of it is firm and coagulated : the coagulated part, however, although opposite to the opinion of Fontana, I view as entirely depending upon a want of
force

force in the poison to destroy the whole of the vital principle, yet destroys so much as to produce death.

This opinion is rendered extremely probable, both from the evident coagulation of the blood in the vessels of most dead animals, and even those who die suddenly, but more particularly from what occurs from the action of the ticunas injected into the veins; there the death is more sudden than from the injection of the viper's poison, and also the consequences more remarkable, all the blood is in a fluid state, deprived of the power of coagulation from air, and the diseased state of certain viscera is far more obvious.

Here, then, we infer from its consequences, the poison of the ticunas more active than the viper's, and consequently from all the blood being in a fluid state, and deprived of the power of coagulation from exposure that it destroys, even in a very small quantity, all the vitality of the blood.

From the weakness of the viper's poison, I would also explain its want of power in killing cold-blooded animals, and not from any thing peculiar in their blood,
only,

only, perhaps, that of being more tenacious of its life ; for we find the ticunas, which evidently acts in the same way, only with greater force, kills them also ; and hence from these circumstances we are led to a contrary conclusion from that of Fontana* ; and instead of saying, that the effects which the viper's venom produce on the blood are more clear and decided than those of the ticunas, from its producing coagulation of it : I would say that this coagulation is a mark of the weakness of the poison, and only a consequence of that poison being inadequate to destroy the whole of the blood's life ; while the fluidity of the blood, its greater tendency to dissolution, and the more obvious changes in certain viscera, evidently shew the greater virulence of the ticunas, and its capability to destroy, in a small quantity, all the blood's life.

Moreover, the opinion which I have ventured to suggest, of the poison of the viper being only destructive to the blood's life in a given quantity, is rendered probable, even from the experiments of Fontana

* Vol. II. p. 134.

himself,

himself, who, following *Mead*, found that the said poison has, when mixed with the blood out of its vessels, only the property of preventing its coagulation when used to a considerable extent *.

The latter case, that is, the effects of the poison on blood drawn from its vessels, tends also much to confirm the idea, both of the mode of action of these poisons, and the power on which they act; for from a set of experiments †, instituted by Fontana, we find that the poison of the viper and ticunas, mixed with the blood, prevent its coagulation, and hasten its putrefaction; which last circumstance, independent of others, is a strong proof that these poisons produce, when used in sufficient quantity, a total annihilation of the blood's life, and have not the effect, as may be supposed, of only rendering the life of the blood insensible to what was formerly a stimulus to it.

We have thus, then, briefly stated our reasons for supposing that these poisons act

* Vol. I. p. 313-377.

† Vol. I. p. 377.

on the living principle of the blood ; owing to the destruction of which, as it is the cause of all the actions which the blood induces, there is a cessation of action, and consequently instant death.

For farther particulars I refer to the Abbe's work itself, replete with much useful information, both as to the mode of action of these poisons, and those medicines which are thought to affect a cure.

To me it appears, that when only a small quantity of the poison is applied, and consequently only slight symptoms ensue, nature herself is the cure, throwing off that quantity of the blood by different ways, which, by the action of these poisons, was rendered inanimate ; perhaps the cure might be somewhat accelerated by transfusion ; but in those cases where the poison has been introduced, in such quantity as to destroy the whole or greater part of the blood's vital power, nothing that I know, nor do I think any thing can effect a cure ; the only probable means would be transfusion ; but here insurmountable bars are presented to our view, as we know of no power by which we can empty the vessels ;
and

and moreover, although we did, and could fill the vessels with healthy blood, yet such a state of very important organs, as the lungs, is so suddenly induced as is inconsistent with life. In vain then we seek for a remedy against this disorder. We may find a preventive, or such a substance as either prevents its admission into the blood ; or although admitted, has the power of so involving or destroying its venomous quality, that from its introduction no bad effects follow.

To one or other of these heads, then, we, without doubt, must refer the influence the lunar caustic shews when mixed with viper's venom, of rendering inert its noxious quality ; for no such power would it shew as a cure, were once the poison allowed to act on the blood.

Thus, then, have we briefly considered the different sections of our definition, and, in my opinion, confirmed them all. Hence can we any longer doubt of the blood's being alive, when we find it governed by fixed and constant laws of living matter, the only test by which we can judge of the presence of life. We have found it sensi-

ble to a stimulus, capable of resisting those hurtful agents which threaten its destruction: and above all, we have found that, by viewing the blood as alive, we can only explain the phenomena of many important functions of the animal body.

The next subject of our enquiry will be, to determine from whence the blood derives this life, by which we are led to our fourth and last general head.

To determine from whence the blood derives its life, is a task no less arduous than involved in obscurity; all we can, perhaps, arrive at, will amount to nothing more than a mere conjecture, and what I offer is as such, rather than the result of experiment or actual observation. The organs which possess the power of rendering the blood living, or changing inanimate to animate matter, are to us not certainly known; but from different circumstances, I think that this change is effected not by any distinct organ, I rather view it as the effect of the whole absorbent and circulating vessels; but, perhaps, some change may also take place in certain glandular organs, with the use of which we are unacquaint-

acquainted; and this the rather, as we shall find the lymphatic glands, which were formerly supposed to perform analogous changes to these organs, possessed of a considerable power of animating the blood.

How these organs which we have enumerated effect the change which we have ascribed to them, or in what this change really consists, we are, perhaps, equally ignorant.

That the first, however, in all probability, depends on the action (specific, we may call it) of vessels, is not improbable, as in other cases we evidently see as wonderful changes effected by them, both in the vegetable and animal kingdom; but in what the other life itself consists I cannot even hesitate a conjecture; it seems to be the most simple of life with which the blood is endowed; but whether the result of a particular organization, or peculiar modification of matter, or on the presence of some invisible matter secreted by either vessels or nerves, or shews those effects from the presence of some immaterial power, is what I cannot pretend to say; perhaps we

shall never be able to ascertain it ; and, at best, more properly a metaphysical than a physiological enquiry. We therefore wave the enquiry, and next, previous to our coming to the main point, as it may be of some service, we will endeavour to ascertain if it resides in any part of the blood more than others.

In what part of the blood life, as we have defined it, more particularly resides is a matter of some doubt ; certain I am, however, from experiment, that it possesses it in the highest degree when all the parts are in their proper proportion, and intimately blended together ; but that one or more parts of it may have it in a greater degree than others, is, I think, from experiments made by freezing, rendered sufficiently probable.

Independent of these, I would even conjecture that this power principally resides in the coagulable lymph ; not that I deny it to the other constituent parts ; for they certainly all of them partake of it in some degree ; but that it more immediately resides in the coagulable lymph, is drawn from our finding that many parts of all
animals

animals have their vessels only of such size as to admit this and serum; in it, or the serum, then, or in both, resides this power, since they produce the effect of the whole.

That the serum has not life, is what I will not pretend to say; but certain am I from experiments by freezing, that it is only in a low degree, and very far inferior to that of the coagulable lymph.

Independent of these, however, what led me to adopt it, is from an observation of the illustrious Haller, who, in endeavouring to render probable the opinion of irritability being independent of sensibility, infers, that the former has its seat in the glutinous matter, connecting the earthy elements of which the fibres are composed; and that this irritability ought to be considered as a peculiar property of this glutinous substance, in like manner as gravity is allowed to be a property of matter in general, although its cause cannot be assigned*.

It is foreign to my purpose to determine, whether or no irritability is independent of

* Aët. Gotting, vol. II. p. 152.

fenfibility ; it is enough for my purpose to acknowledge, that it appears to me highly probable, from many experiments and observations lately brought forward by many ingenious men. Taking it for granted, then, that there exists a power in muscles called irritability, independent and distinct from fenfibility, it remains for me to hazard a conjecture concerning its formation.

Dr. Haller affirms, and I believe it exists in truth, that this irritability resides in the glutinous part of the muscle ; and that this glutinous part is nothing but the gluten of the blood unchanged, only applied, deposited, or particularly arranged, by the arteries, appears to me no improbable supposition. The glutinous matter of both, in a dead state, is exactly the same.

So far, then, from saying that the irritability of this glutinous matter is a peculiar property of it, as gravity is of matter in general, which, for several reasons stated by Dr. White *, cannot be admitted, I would say, that as we have demonstrated simple life, or life, at least, unconnected

* White's works, p. 294.

with any visible organization, to exist in the blood, that from the glutinous part of this blood, applied and arranged by the arteries in a particular manner, it forms the glutinous part of the muscular fibre; and that the power this fibre has of shortening itself, when acted upon by a stimulus, does not depend on the life it possesses, being different from that which it was when gluten circulating in the blood, but entirely from a different organization, which is certainly sufficient to explain all the phenomena.

Dr. White employs the same reasoning, to shew how the nerves are not irritable, and which is equally applicable to every different kind of constituent part of the animal body. He says, “ Since muscles
 “ are the only organs of the body, which
 “ by their peculiar structure are fitted for
 “ motion, it is no wonder the nerves
 “ should have no irritability, since the
 “ want of it is only a necessary consequence of their make *.”

* White's works, p. 280.

On the principle we have laid down, we also easily explain the irritability of many plants ; and this, when seriously considered, (which I cannot do at present) serves to render the opinion about the formation of irritability not a little probable.

Then it is evident I consider the life of the blood as more immediately residing in its gluten. To prove this, I at present have contented myself with observation ; yet at a future period actual experiment, I trust, will confirm it.

That that property of the blood, which we have denominated its life, may result from the action of those organs which we have marked out, is an opinion which, from analogy, we can at least render highly probable ; after which we will use a few additional arguments to shew that its life is not secreted by the nerves.

Previous to considering the change which they induce, we will take a short view of the process instituted by nature, for rendering inanimate matter capable of being acted upon by that power which gives it the property of animated matter.

Mr.

Mr. John Hunter was the first who satisfactorily pointed out how and by what means this change was induced: he made it appear, that it was from the action of a peculiar fluid poured into the stomach.

This opinion future observers have sufficiently confirmed; but what the change is which this fluid, perhaps in conjunction with others, produce, is what we are entirely ignorant of, corresponding with no process with which we are acquainted.

The conjecture I should make on this subject, were I allowed, would be, that from the action of this peculiar fluid on, or from its mixture with, inanimate matter, it is thereby rendered a fit pabulum for being acted upon by the lymphatic system, in which a farther change is induced, by which it is rendered capable of being farther acted upon by the blood vessels, in which it is evident a farther and very important change is affected.

This opinion seems to be rendered not a little probable, from what we see happen in several particular cases, many substances resisting the action of this fluid, by which they sometimes cannot get into the system,

unless from a minute mechanical division of their particles, which, when it happens, always a morbid state of some function is induced from their acting as dead matter on the living solids : and hence to conclude this part and return, we say, that as we consider blood the pabulum of other secretions, so do we the result of the process of digestion, the only fit pabulum for being absorbed and acted upon by the lymphatic glands and vessels, the product of which is chyle, and which I view as a secretion ; and as a proof of this, we need only observe the colour of this fluid in a lacteal near its origin, and at its termination, after having passed through many glands ; there the difference is great, both as to colour, consistence, the effects of air, &c.

Moreover, unless we ascribe to the lymphatic glands a power of changing this matter circulated through them, what use shall be ascribed to them : that of Hewson seems now neglected, while the same fate, I hope, awaits that opinion which supposes them a set of guards placed, as it were, to hinder the admission of hurtful bodies into our system. But it appears to me nature
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in their formation had no such end in view. It is only with some of the hurtful bodies that they perform this office, and then they commonly do more harm than good ; for supposing them for a moment possessed of this power, what is to become of the matter which they thus stop in its progress ? it is evident from the number of valves that they cannot force it back ; and the only way we could suppose it to be thrown off, would be, from the induction of inflammation and its consequences ; but it is at once evident, the remedy, on many accounts, would be worse than the disease ; and from these occurrences the danger would be increased.

For any thing we know to the contrary, then, the lymphatic glands may perform this function. I have been told, that an eminent London anatomist views them in this light. This power, which I attribute to these glands and vessels, may appear not a little extraordinary ; yet from analogy I think it may be rendered highly probable.

Dr. Hendy has, I think, in his excellent essay on glandular secretion, clearly pointed it

it out, that the various changes which the glands induce result entirely from the action of the vessels. It would exceed the limits of this essay to even attempt an analysis of them; besides, the doctrine is now generally received which renders it the less necessary. We may only observe, that from the analogy of vegetables, from none of the secretions being found in the blood, and from the effects of the passions on secretion, we take it for granted, that it depends on the specific action of vessels.

To draw our analogy, then, the *data* being fixed, let us remark, that nervous energy, many, not without good reason, suppose to be secreted in the brain; even the opponents of life being attached to fluids, may we not suppose the life of the blood also secreted by vessels?

An analogy more applicable, however, may be drawn from what we see happen in that secretion by the testicle of the male, which we find has the property when mixed, or applied to that matter secreted by the ovaria of the female, of making a complete animal.

Hence,

Hence, since we find that from the specific action of vessels matters are formed which, when they come in contact in certain situations, form a complete animal; may we not with probability conclude, that also from another specific action of vessels results that property which we call life in the blood?

That this life, however, is not the result of these vessels alone, that is, the lymphatics, is not a little probable from those effects which we see follow its admission into what are called the blood vessels; there it becomes more elaborate, and assumes, from a white, a red colour; how these red particles are formed, and of what use they are in the mass, we are as yet ignorant. Certain I am, that by experiment it will be found that the blood, in this state, shews the greatest signs of life: and hence may we not infer, that those vessels which circulate it have a power of contributing to the support of this life.

This is not a little probable, not only from those arguments which we have already used, but also from those wonderful changes which Mr. Hewson observed they
had

had on the blood. To them, then, I think it more than probable we must attribute the power of perfecting that which was begun in the absorbent system; which change must be previously affected.

That this change is necessary previous to the entrance of chyle into the veins, is rendered probable from what we see happen when we inject fluids as bland, and nearly of the same appearance and constituent parts as the chyle, even in small quantities, directly into the veins; there they induce the most distressing symptoms, terminating in many cases fatally; which, I think, never would occur, had not the lymphatic system the power of inducing on the chyle some change, rendering it different from inanimate matter, since we see no effects follow its admission into the veins.

This mode of explaining the change induced on the blood I thus adopt from the nature of the specific actions of vessels; but we are still ignorant how they effect this change, and equally so with that of the specific action, either in glands in which the product of the action is obvious to our
I senses,

senses, or in general morbid actions, from the application of any contagion. In all, however, we see the effect, and our ignorance how the cause induces the effect weighs nothing, when we consider the state of human science.

Moreover I was led to adopt this opinion, from its being, to me, the most probable of any which I could fancy. It occurred to me that animation might take place in the lungs, and also that it might be a consequence of a principle secreted by the nerves, and poured into the vessels from their extremities. Innumerable and unsurmountable objections, however, soon occurred to these hypotheses; a few of which I shall mention, and with this conclude the present essay.

That in the lungs a change is induced; is sufficiently obvious, and that, too, highly necessary to life; yet, for several reasons, I think it not improbable that this change consists in something thrown out, and not taken in. If we thus view respiration, it is obvious we rank the lungs among excretory organs, and that by the large extent

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of

of surface in them something very destructive to animal life is thrown off, and which is the cause of animals so soon dying from its suppression; but then from a suppression of any of the other excretions, death as inevitably ensues as from it; only from the matter thrown off being less destructive, death is less sudden.

This view I take of the matter, because I cannot see any thing like life in air; and that life in the blood only consists in simple mixture with a fluid secreted by the nerves, is, I think, not more probable; for were not some change induced on chyle, previous to its entrance into the vessels, which could only be effected by its being subjected to the action of these vessels, we certainly should never find any danger result from the injection of bland fluids into the red vessels, if simple mixture was all that was necessary, as they would be so immediately mixed with a large mass of animated matter, that, did not life result from the specific action of vessels, no bad effects would follow. Besides this, the knife of the anatomist can demonstrate few or no nerves termina-
ting

ting in the blood vessels ; nor is this matter, so said to be secreted, itself more obvious : both of which, at least the former, would certainly occur, were the animation of such a large mass of matter owing to that cause.

Thus I conclude this essay, deeming it unnecessary to apologise farther for the many loose hints which I have thrown out, and the feeble attempts I have made to overthrow theories sanctioned by great names, and pretty generally received—relying on the candour and liberality of a judicious public, ever ready to treat with lenity the errors of those who even endeavour to extend our knowledge, and to give them that praise due to a laudable inclination, although the state of their abilities or opportunities may have been such as to deny them the praise of considerable progress.

The candid public will also, I trust, excuse me, when I seize upon this opportunity of publicly returning my sincere thanks to Mr. James Pearson, of Ayr, for
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the many singular favours and ingenious communications with which he has been pleased to honour me, and that too with no less politeness than generosity.

THE END.





